NEW EXPERIMENTS AND OBSERVATIONS ON ELECTRICITY.

MADE AT

Philadelphia in *America.*

BY

BENJAMIN FRANKLIN, *Esq;*

AND

Communicated in several Letters to P. COLLINSON, *Esq;* of *London*, F.R.S.

PART II.

The THIRD EDITION.

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LETTER V.

FROM

BENJAMIN FRANKLIN, *Esq*; of PHILADELPHIA.

TO

PETER COLLINSON, *Esq*; F. R. S. *at* London.

SIR, *July* 27, 1750.

MR *W-tſ-n*, I believe, wrote his obſervations on my laſt paper in haſte, without having firſt well conſidered the experiments related §. 17. which ſtill appear to me deciſive in the queſtion,—*Whether the accumulation of the electrical fire be in the electrified glaſs, or in the non-electric matter connected with the glaſs?* And to demonſtrate that 'tis really in the glaſs.

As to the experiment that ingenious gentleman mentions, and which he thinks concluſive on the other ſide, I perſuade myſelf he will change his opinion of it, when he conſiders, that as one perſon applying the wire of the charged bottle to warm ſpirits, in a ſpoon held by another perſon, both ſtanding on the floor, will fire the ſpirits, and yet ſuch firing will not determine whether the accumula-

tion was in the glaſs or the non-electric; ſo the placing another perſon between them, ſtanding on wax, with a baſon in his hand, into which the water from the phial is pour'd, *while he at the inſtant of pouring* preſents a finger of his other hand to the ſpirits, does not at all alter the caſe; the ſtream from the phial, the ſide of the baſon, with the arms and body of the perſon on the wax, being all together but as one long wire, reaching from the internal ſurface of the phial to the ſpirits.

June 29, 1751. In Capt. *Waddel*'s account of the effects of lightening on his ſhip, I could not but take notice of the large comazants (as he calls them) that ſettled on the ſpintles at the topmaſt heads, and burnt like very large torches (before the ſtroke). According to my opinion, the electrical fire was then drawing off, as by points, from the cloud; the largeneſs of the flame betokening the great quantity of electricity in the cloud: and had there been a good wire communication from the ſpintle heads to the ſea, that could have conducted more freely than tarred ropes, or maſts of turpentine wood, I imagine there would either have been no ſtroke; or, if a ſtroke, the wire would have conducted it all into the ſea without damage to the ſhip.

His compaſſes loſt the virtue of the load-ſtone, or the poles were revers'd; the North point turning to the South. —By electricity we have (*here* at *Philadelphia*) frequently given polarity to needles, and reverſed them at pleaſure.

Mr

Mr *Wilſon*, at *London*, tried it on two large maſſes, and with too ſmall force.

A ſhock from four large glaſs jars, ſent through a fine ſewing needle, gives it polarity, and it will traverſe when laid on water.—If the needle when ſtruck lies Eaſt and Weſt, the end entered by the electric blaſt points North. —If it lies North and South, the end that lay towards the North will continue to point North when placed on water, whether the fire entered at that end, or at the contrary end.

The polarity given is ſtrongeſt when the needle is ſtruck, lying North and South, weakeſt when lying Eaſt and Weſt; perhaps if the force was ſtill greater, the South end, enter'd by the fire, (when the needle lies North and South) might become the North, otherwiſe it puzzles us to account for the inverting of compaſſes by lightening; ſince their needles muſt always be found in that ſituation, and by our little experiments, whether the blaſt entered the North and went out at the South end of the needle, or the contrary, ſtill the end that lay to the North ſhould continue to point North.

In theſe experiments the ends of the needles are ſometimes finely blued like a watch ſpring by the electric flame. —This colour given by the flaſh from two jars only, will wipe off, but four jars fix it, and frequently melt the needles. I ſend you ſome that have had their heads and points melted off, by our mimic lightning; and a pin that had its point melted off, and ſome part of its head

and

and neck run. Sometimes the ſurface on the body of the needle is alſo run, and appears bliſter'd when examin'd by a magnifying glaſs: the jars I make uſe of hold 7 or 8 gallons, and are coated and lined with tin-foil; each of them takes a thouſand turns of a globe nine inches diameter to charge it.

I ſend you two ſpecimens of tin-foil melted between glaſs, by the force of two jars only.

I have not heard that any of your *European* electricians have hitherto been able to fire gunpowder by the electric flame.—We do it here, in this manner.—A ſmall cartridge is filled with dry powder, hard rammed, ſo as to bruiſe ſome of the grains, two pointed wires are then thruſt in, one at each end, the points approaching each other in the middle of the cartridge till within the diſtance of half an inch, then the cartridge being placed in the circle, when the four jars are diſcharged, the electric flame leaping from the point of one wire to the point of the other, within the cartridge amongſt the powder, *fires it*, and the exploſion of the powder is at the ſame inſtant with the crack of the diſcharge.

Yours, &c.

B. FRANKLIN.

LET-

LETTER VI.

FROM

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*.

TO

C. C. Esq; at *New York*.

SIR, 1751.

I Incloſe you Anſwers, ſuch as my preſent hurry of buſineſs will permit me to make, to the principal queries contained in yours of the 28th inſtant, and beg leave to refer you to the latter piece in the printed collection of my papers, for farther explanation of the difference between what are called *electrics per ſe* and *non electrics*. When you have had time to read and conſider theſe papers, I will endeavour to make any new experiments you ſhall propoſe, that you think may afford farther light or ſatisfaction to either of us; and ſhall be much obliged to you for ſuch remarks, objections, &c. as may occur to you.—I forget whether I wrote you that I have melted braſs pins, and ſteel needles, inverted the poles of the magnetic needle, given a magnetiſm and polarity to needles

needles that had none, and fired dry gunpowder by the electric ſpark. I have five bottles that contain 8 or 9 gallons each, two of which charg'd, are ſufficient for thoſe purpoſes; but I can charge and diſcharge them all together. There are no bounds (but what expence and labour give) to the force man may raiſe and uſe in the electrical way: For bottle may be added to bottle in infinitum, and all united and diſcharged together as one, the force and effect proportioned to their number and ſize. The greateſt known effects of common lightening, may, I think, without much difficulty be exceeded in this way, which a few years ſince could not have been believed, and even now may ſeem to many a little extravagant to ſuppoſe.—So we are got beyond the ſkill of *Rabelais*'s devils of two years old, who, he humouroufly ſays, had only learnt to thunder and lighten a little round the head of a cabbage.

I am, with ſincere reſpect,

Yotr moſt obliged humble ſervant.

B. FRANKLIN.

Que-

Queries *and* Anſwers, *referr'd to in the foregoing Letter.*

Query, Wherein conſiſts the difference between an *electric* and a *non-electric* body?

Anſwer. The terms electric *per ſe*, and non-electric were firſt uſed to diſtinguiſh bodies, on a miſtaken ſuppoſition that thoſe called electrics *per ſe* alone contained electric matter in their ſubſtance, which was capable of being excited by friction, and of being produced or drawn from them and communicated to thoſe called non-electrics, ſuppoſed to be deſtitute of it: For the glaſs, &c. being rubbed, diſcover'd ſigns of having it, by ſnapping to the finger, attracting, repelling, &c. and could communicate thoſe ſigns to metals and water.—Afterwards it was found, that rubbing of glaſs would not produce the electric matter, unleſs a communication was preſerved between the rubber and the floor; and ſubſequent experiments prov'd that the electric matter was really drawn from thoſe bodies that at firſt were thought to have none in them. Then it was doubted whether glaſs and other bodies called *electrics per ſe* had really any electric matter in them, ſince they apparently afforded none but what they firſt extracted from thoſe which had been called non-electrics. But ſome of my experiments ſhew that glaſs contains it in great quantity, and I now ſuſpect it to be pretty equally diffuſed in all the matter of this terraqueous

globe. If ſo, the terms *electric per ſe*, and *non electric*, ſhould be laid aſide as improper: And (the only difference being this, that ſome bodies will conduct electric matter, and others will not) the terms *conductors* and *non-conductors* may ſupply their place. If any portion of electric matter is applied to a piece of conducting matter, it penetrates and flows through it, or ſpreads equally on its ſurface; if applied to a piece of non-conducting matter, it will do neither. Perfect conductors of electric matter are only metals and water. Other bodies conducting only as they contain a mixture of thoſe; without more or leſs of which they will not conduct at all.* This (by the way) ſhews a new relation between metals and water heretofore unknown.

To illuſtrate this by a compariſon, which, however, can only give a faint reſemblance. Electric matter paſſes through conductors as water paſſes through a porous ſtone, or ſpreads on their ſurfaces as water ſpreads on a wet ſtone; but when apply'd to non-conductors, 'tis like water dropt on a greaſy ſtone; it neither penetrates, paſſes through, nor ſpreads on the ſurface, but remains in drops where it falls. See farther on this head in my laſt printed piece.

Query. What are the effects of air in electrical experiments.

Anſwer. All I have hitherto obſerved are theſe. Moiſt air

* This propoſition is ſince found to be too general; Mr *Wilſon* having diſcovered that melted wax and roſin will alſo conduct.

air receives and conducts the electrical matter in proportion to its moiſture, quite dry air not all: air is therefore to be claſs'd with the non-conductors. Dry air aſſiſts in confining the electrical atmoſphere to the body it ſurrounds, and prevents its diſſipating: for in vacuo it quits eaſily, and points operate ſtronger, *i. e.* they throw off or attract the electrical matter more freely, and at greater diſtances; ſo that air intervening obſtructs its paſſing from body to body, in ſome degree. A clean electrical phial and wire, containing air inſtead of water, will not be charged nor give a ſhock, any more than if it was fill'd with powder of glaſs; but exhauſted of air it operates as well as if fill'd with water. Yet, an electrical atmoſphere and air do not ſeem to exclude each other, for we breath freely in ſuch an atmoſphere, and dry air will blow through it without diſplacing or driving it away. I queſtion whether the ſtrongeſt dry N. Weſter would diſſipate it. I once electrified a large cork ball, at the end of a ſilk thread 3 feet long, the other end of which I held in my fingers, and whirl'd it round, like a ſling, 100 times in the air, with the ſwifteſt motion I could poſſibly give it, yet it retained its electrical atmoſphere, though it muſt have paſs'd through 800 yards of air, allowing my arm in giving the motion to add a foot to the ſemi-diameter of the circle.—By quite dry air, I mean the dryeſt we have: for perhaps we never have any perfectly free from moiſture. An electrical atmoſphere raiſed round a thick wire, inſerted in a phial of air, drives out none of the air, nor

on withdrawing that atmoſphere will any air ruſh in, as I have found by a very curious experiment, accurately made, whence we concluded that the air's elaſticity was not affected thereby.

An Experiment towards diſcovering more of the Qualities of the Electric Fluid.

FROM the prime conductor, hang a bullet by a wire hook; under the bullet at half an inch diſtance, place a bright piece of ſilver to receive the ſparks; then let the wheel be turned, and in a few minutes (if the repeated ſparks continually ſtrike in the ſame ſpot) the ſilver will receive a blue ſtain near the colour of a watch ſtring.

A bright piece of iron will alſo be ſpotted, but not with that colour; it rather ſeems corroded.

On gold, braſs, or tin, I have not perceived that it makes any impreſſion. But the ſpots on the ſilver or iron will be the ſame, whether the bullet be lead, braſs, gold, or ſilver.

On a ſilver bullet, there will alſo appear a ſmall ſpot, as well as on the plate below it.

LET-

LETTER VII.

FROM

Mr E. KINNERSLEY, at *Boſton*,

TO

BENJAMIN FRANKLIN, Eſq; at *Philadelphia*.

SIR *Feb.* 3, 1752.

I Have the following experiments to communicate: I held in one hand a wire, which was faſten'd at the other end to the handle of a pump, in order to try whether the ſtroke from the prime conductor, through my arms, would be any greater, than when convey'd only to the ſurface of the earth, but could diſcover no difference.

I placed the needle of a compaſs on the point of a long pin, and holding it in the atmoſphere of the prime conductor, at the diſtance of about three inches, found it to whirl round, like the flyers of a jack, with great rapidity.

I ſuſpended with ſilk, a cork ball, about the bigneſs of a pea, and preſented to it, rubbed amber, ſealing wax, and ſulphur, by each of which it was ſtrongly repelled; then

then I tried rubbed glaſs and china, and found that each of theſe would attract it, until it became electrified again, and then it would be repelled as at firſt; and while thus repelled by the rubbed glaſs or china, either of the others when rubbed would attract it. Then I electrified the ball, with the wire of a charged phial, and preſented to it rubbed glaſs (the ſtopper of a decanter) and a china tea cup, by which it was as ſtrongly repelled, as by the wire; but when I preſented either of the other rubbed electrics, it would be ſtrongly attracted, and when I electrified it, by either of theſe, till it became repelled, it would be attracted by the wire of the phial, but be repelled by its coating.

Theſe experiments ſurprized me very much, and have induced me to infer the following parodoxes.

1. If a glaſs globe be placed at one end of a prime conductor, and a ſulphur one at the other end, both being equally in good order, and in equal motion, not a ſpark of fire can be obtain'd from the conductor; but one globe will draw out, as faſt as the other gives in.

2. If a phial be ſuſpended on the conductor, with a chain from its coating to the table, and only one of the globes be made uſe of at a time, 20 turns of the wheel, for inſtance, will charge it; after which, ſo many turns of the other wheel will diſcharge it; and as many more will charge it again.

3. The globes being both in motion, each having a ſeparate conductor, with a phial ſuſpended on one of them,

and

and the chain of it faſtened to the other, the phial will become charged; one globe charging poſitively, the other negatively.

4. The phial being thus charged, hang it in like manner on the other conductor; ſet both wheels a going again, and the ſame number of turns that charged it before, will now diſcharge it; and the ſame number repeated, will charge it again.

5. When each globe communicates with the ſame prime conductor, having a chain hanging from it to the table, one of them, when in motion, (but which I can't ſay) will draw fire up through the cuſhion, and diſcharge it through the chain; the other will draw it up through the chain, and diſcharge it through the cuſhion.

I ſhould be glad if you would ſend to my houſe for my ſulphur globe, and the cuſhion belonging to it, and make the trial; but muſt caution you not to uſe chalk on the cuſhion, ſome fine powdered ſulphur will do better. If, as I expect, you ſhould find the globes to charge the prime conductor differently, I hope you will be able to diſcover ſome method of determining which it is that charges poſitively.

I am, &c.

E. KINNERSLEY.

LETTER VIII.

FROM

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*,

TO

Mr E. KINNERSLEY, at *Boston*.

SIR, *March* 2, 1752.

I Thank you for the experiments communicated. I sent immediately for your brimstone globe, in order to make the trials you desired, but found it wanted centers, which I have not time now to supply; but the first leisure I will get it fitted for use, try the experiments, and acquaint you with the result.

In the mean time I suspect, that the different attractions and repulsions you observed, proceeded rather from the greater or smaller quantities of the fire you obtained from different bodies, than from its being of a different *kind*, or having a different *direction*. In haste,

I am, &c.

B. FRANKLIN.

LET-

LETTER IX.

FROM

BENJAMIN FRANKLIN, *Esq*; of PHILADELPHIA.

TO

To Mr E. KINNERSLEY, at *Boston*.

SIR, *March* 16, 1752.

HAving brought your brimstone globe to work, I try'd one of the experiments you proposed, and was agreeably surprized to find, that the glass globe being at one end of the conductor, and the sulphur globe at the other end, both globes in motion, no spark could be obtained from the conductor, unless when one globe turned slower, or was not in so good order as the other; and then the spark was only in proportion to the difference, so that turning equally, or turning that slowest which work'd best, would again bring the conductor to afford no spark.

I found also, that the wire of a phial charg'd by the glass globe, attracted a cork ball that had touch'd the wire of a phial charged by the brimstone globe, and *vice versa*,

ſo that the cork continued to play between the two phials, juſt as when one phial was charged through the wire, the other through the coating, by the glaſs globe alone. And two phials charged, the one by the brimſtone globe, the other by the glaſs globe, would be both diſcharged by bringing their wires together, and ſhock the perſon holding the phials.

From theſe experiments, one may be certain that your 2d, 3d, and 4th propoſed experiments, would ſucceed exactly as you ſuppoſe, though I have not tried them, wanting time.—I imagine it is the glaſs globe that charges poſitively, and the ſulphur negatively, for theſe reaſons, 1. Though the ſulphur globe ſeems to work equally well with the glaſs one, yet it can never occaſion ſo large and diſtant a ſpark between my knuckle and the conductor when the ſulphur one is working, as when the glaſs one is uſed; which, I ſuppoſe, is occaſioned by this, that bodies of a certain bigneſs cannot ſo eaſily part with a quantity of electrical fluid they have and hold attracted *within* their ſubſtance, as they can receive an additional quantity *upon* their ſurface by way of atmoſphere. Therefore ſo much cannot be drawn *out* of the conductor, as can be thrown *on* it. 2. I obſerve that the ſtream or bruſh of fire appearing at the end of a wire connected with the conductor, is long, large, and much diverging, when the glaſs globe is uſed, and makes a ſnapping (or rattling) noiſe: but when the ſulphur one is uſed, it is ſhort, ſmall, and makes a hiſſing noiſe; and juſt the reverſe

verſe of both happens, when you hold the ſame wire in your hand, and the globes are worked alternately: the bruſh is large, long, diverging and ſnapping (or rattling) when the ſulphur globe is turn'd; ſhort, ſmall, and hiſſing when the glaſs globe is turn'd.---When the bruſh is long, large, and much diverging, the body to which it joins, ſeems to me to be throwing the fire out; and when the contrary appears, it ſeems to be drinking in. 3. I obſerve, that when I held my nuckle before the ſulphur globe, while turning, the ſtream of fire between my knuckle and the globe, ſeems to ſpread on its ſurface, as if it flowed from the finger; on the glaſs globe 'tis otherwiſe. 4. The cool wind (or what was called ſo) that we uſed to feel as coming from an electrified point, is much more ſenſible, when the glaſs globe is uſed, than when the ſulphur one.---But theſe are haſty thoughts. As to your fifth paradox, it muſt likewiſe be true, if the globes are alternately worked; but if work'd together, the fire will neither come up nor go down by the chain, becauſe one globe will drink it as faſt as the other produces it.

I ſhould be glad to know whether the effects would be contrary, if the glaſs globe is ſolid, and the ſulphur globe is hollow; but I have no means at preſent of trying.

In your journeys, your glaſs globes meet with accidents, and ſulphur ones are heavy and inconvenient. *Query.* Would not a thin plane of brimſtone, caſt on a board, ſerve on occaſion as a cuſhion, while a globe of leather

ſtuffed

ſtuffed (properly mounted) might receive the fire from the ſulphur, and charge the conductor poſitively? Such a globe would be in no danger in breaking. I think I can conceive how it may be done; but have not time to add more than that I am,

Yours, &c.

B. FRANKLIN.

LETTER X.

FROM

BENJAMIN FRANKLIN, *Eſq*; of *Philadelphia.*

Oct. 19, 1752.

AS frequent mention is made in the news papers from *Europe*, of the ſucceſs of the *Philadelphia* experiment for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, *&c.* it may be agreeable to inform the curious that the ſame experiment has ſucceeded in *Philadelphia*, though made in a different and more eaſy manner, which is as follows:

Make a ſmall croſs of two light ſtrips of cedar, the arms

arms ſo long as to reach to the four corners of a large thin ſilk handkerchief when extended ; tie the corners of the handkerchief to the extremities of the croſs, ſo you have the body of a kite ; which being properly accommodated with a tail, loop, and ſtring, will riſe in the air, like thoſe made of paper ; but this being of ſilk, is fitter to bear the wind and wet of a thunder guſt without tearing. To the top of the upright ſtick of the croſs is to be fixed a very ſharp pointed wire, riſing a foot or more above the wood. To the end of the twine, next the hand, is to be ty'd a ſilk ribbon, and where the ſilk and twine join, a key may be faſtened. This kite is to be raiſed when a thunder guſt appears to be coming on, and the perſon who holds the ſtring muſt ſtand within a door, or window, or under ſome cover, ſo that the ſilk ribbon may not be wet ; and care muſt be taken that the twine does not touch the frame of the door or window. As ſoon as any of the thunder clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified, and the looſe filaments of the twine will ſtand out every way, and be attracted by an approaching finger. And when the rain has wet the kite and twine, ſo that it can conduct the electric fire freely, you will find it ſtream out plentifully from the key on the approach of your knuckle. At this key the phial may be charged ; and from electric fire thus obtained, ſpirits may be kindled, and all the other electric experiments be performed, which are uſually done by the

help

help of a rubbed glaſs globe or tube; and thereby the ſameneſs of the electric matter with that of lightening completely demonſtrated.

B. F.

LETTER XI.

FROM

BENJAMIN FRANKLIN, *Eſq*; of *Philadelphia*,

AS you tell me our friend *Cave* is about to add ſome later experiments to my pamphlet, with the *Errata*, I ſend a copy of a letter from Dr *Colden*, which may help to fill a few pages; alſo my kite experiment in the *Penſylvania Gazette*: to which I have nothing new to add, except the following experiment towards diſcovering more of the qualities of the electric fluid. From the prime conductor, hang a bullet by a wire hook; under the bullet, at half an inch diſtance, place a bright piece of ſilver to receive the ſparks; then let the wheel be turned, and in a few minutes, (if the repeated ſparks continually ſtrike in the ſame ſpot) the ſilver will receive a blue ſtain near the colour of a watch ſpring. A bright piece of iron will alſo be ſpotted, but not

not with that colour: it rather ſeems to be corroded. On gold, braſs, or tin, I have not perceived that it makes any impreſſion. But the ſpots on the ſilver or iron will be the ſame, whether the bullet be lead, braſs, gold, or ſilver. On a ſilver bullet there will alſo appear a ſmall ſpot, as well as in the plate below it.

F I N I S.

ADDITIONAL ERRATA *in* PART I. *to the printed* ELECTRICAL PAPERS. *By* B. FRANKLIN.

PAGE 38, line 2. read *repreſented by* A and B, *Fig.* 6.

Page 40, line 7. r. *repulſion.*

Page 65, l. 19. r. *Aqua Regia.*

Page 70, l. 6. r. *rubbed.*

Fig. VI. in the plate, wants the referring letters A, B, C, D. In the copy that Fig. was mentioned as a profile of a piece of water, the little circles repreſenting particles. The two upper circles were mark'd A and B, and two others in the under line or row C and D. As it ſtands 'tis wholly unintelligible.

Page 73, l. 17. r. *air.*

Page 74, l. 7. r. *electrical.*

Page 81, l. 3. place a *comma* at conduct, and dele *comma* at floor.

Page 85, l. 5. r. *diſcharged.* l. 13, r. *p.* 24.

Page 86, l. 20. r. *after one.*

NEW EXPERIMENTS

AND

OBSERVATIONS

ON

ELECTRICITY.

MADE AT

Philadelphia in *America.*

BY

BENJAMIN FRANKLIN, *Esq;*

Communicated to P. COLLINSON, *Esq;* of *London*, F.R.S.

And read at the Royal Society *June* 27, and *July* 4, 1754.

To which are added

A Paper on the same Subject by *J. Canton*, M.A. F.R.S. and read at the Royal Society *Dec.* 6, 1753; and another in defence of Mr *Franklin* against the Abbe *Nollet*, by Mr *D. Colden*, of *New York.*

PART III.

LONDON:

Printed and sold by D. HENRY, and R. CAVE, at *St John's-Gate.* 1754. (Price 1 *s.*)

LETTER XII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

TO

PETER COLLINSON, *Esq*; F. R. S. *London*.

SIR, *Philadelphia, September* 1753.

IN my former paper on this subject, wrote first in 1747, enlarged and sent to *England* in 1749, I considered the sea as the grand source of lightning; imagining its luminous appearance to be owing to electric fire, produc'd by friction between the particles of water and those of salt. Living far from the sea, I had then no opportunity of making experiments on the sea water, and so embrac'd this opinion too hastily.

For in 1750 and 1751, being occasionally on the sea coast, I found, by experiments, that sea water in a bottle, tho' at first it would by agitation appear luminous, yet in a few hours it lost that virtue; *hence, and from this*, that I could not by agitating a solution of sea salt in water pro-

duce any light, I firſt began to doubt of my former hypotheſis, and to ſuſpect that the luminous appearance in ſea water muſt be owing to ſome other principles.

I then conſidered whether it were not poſſible, that the particles of air, being electrics *per ſe*, might, in hard gales of wind, by their friction againſt trees, hills, buildings, *&c.* as ſo many minute electric globes, rubbing againſt non-electric cuſhions, draw the electric fire from the earth; and that the riſing vapours might receive that fire from the air, and by ſuch means the clouds become electrified.

If this were ſo, I imagined that by forcing a conſtant violent ſtream of air againſt my prime conductor, by bellows, I ſhould electrify it *negatively*; the rubbing particles of air, drawing from it part of its natural quantity of the electric fluid. I accordingly made the experiment, but it did not ſucceed.

In *September* 1752, I erected an iron rod to draw the lightning down into my houſe, in order to make ſome experiments on it, with two bells to give notice when the rod ſhould be electrify'd: A contrivance obvious to every electrician.

I found the bells rang ſometimes when there was no lightning or thunder, but only a dark cloud over the rod; that ſometimes after a flaſh of lightning they would ſuddenly ſtop; and at other times, when they had not rang before, they would, after a flaſh, ſuddenly begin to ring; that the electricity was ſometimes very faint, ſo that when a ſmall

a ſmall ſpark was obtain'd, another could not be got for ſometime after; at other times the ſparks would follow extremely quick, and once I had a continual ſtream from bell to bell, the ſize of a crow-quill: Even during the ſame guſt there were conſiderable variations.

In the winter following I conceived an experiment, to try whether the clouds were electrify'd *poſitively*, or *negatively*; but my pointed rod, with its apparatus, becoming out of order, I did not refit it till towards the ſpring, when I expected the warm weather would bring on more frequent thunder-clouds.

The experiment was this: To take two phials; charge one of them with lightning from the iron rod, and give the other an equal charge by the electric glaſs globe, thro' the prime conductor: When charg'd, to place them on a table within three or four inches of each other, a ſmall cork ball being ſuſpended by a fine ſilk thread from the ceiling, ſo as it might play between the wires. If both bottles then were electrifyed *poſitively*, the ball being attracted and repell'd by one, muſt be alſo repell'd by the other. If the one *poſitively*, and the other *negatively*; then the ball would be attracted and repell'd alternately by each, and continue to play between them as long as any conſiderable charge remained.

Being very intent on making this experiment, it was no ſmall mortification to me, that I happened to be abroad during two of the greateſt thunder-ſtorms we had early in the ſpring; and tho' I had given orders in my family, that

if the bells rang when I was from home, they ſhould catch ſome of the lightning for me in electrical phials, and they did ſo; yet it was moſtly diſſipated before my return; and in ſome of the other guſts, the quantity of lightning I was able to obtain, was ſo ſmall, and the charge ſo weak, that I could not ſatisfy myſelf: Yet I ſometimes ſaw what heighten'd my ſuſpicious and inflam'd my curioſity.

At laſt, on the 12th of *April* 1753, there being a ſmart guſt of ſome continuance, I charg'd one phial pretty well with lightning, and the other equally, as near as I could judge, with electricity from my glaſs globe; and having plac'd them properly, I beheld, with great ſurprize and pleaſure, the cork ball play briſkly between them; and was convinc'd that one bottle was electriſed *negatively*.

I repeated this experiment ſeveral times during the guſt, and in eight ſucceeding guſts, always with the ſame ſucceſs; and being of opinion (for reaſons I formerly gave in my letter to Mr *Kinnerſley*, ſince printed in *London*) that the glaſs globe electriſes *poſitively*; I concluded that the clouds are *always* electriſed *negatively*, or have always in them leſs than their natural quantity of the electric fluid.

Yet notwithſtanding ſo many experiments, it ſeems I concluded too ſoon; for at laſt, *June* the 6th, in a guſt which continued from five o'clock, P. M. to 7, I met with one cloud that was electriſed poſitively, tho' ſeveral that paſs'd over my rod before, during the ſame guſt, were in the negative ſtate. This was thus diſcovered:

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I had another concurring experiment, which I often repeated, to prove the negative ſtate of the clouds, *viz.* While the bells were ringing, I took the phial charg'd from the glaſs globe, and apply'd its wire to the erected rod; conſidering, that if the clouds were electrified *poſitively*, the rod, which received its electricity from them, muſt be ſo too; and then the additional *poſitive* electricity of the phial would make the bells ring faſter:—But, if the clouds were in a *negative* ſtate, they muſt exhauſt the electric fluid from my rod, and bring that into the ſame negative ſtate with themſelves; and then the wire of a poſitively charg'd phial, ſupplying the rod with what it wanted (which it was oblig'd otherwiſe to draw from the earth by means of the pendulous braſs ball playing between the two bells) the ringing would ceaſe till the bottle was diſcharg'd.

In this manner I quite diſcharged into the rod ſeveral phials that were charged from the glaſs globe, the electric fluid ſtreaming from the wire to the rod, 'till the wire would receive no ſpark from the finger; and during this ſupply to the rod from the phial, the bells ſtopt ringing; but by continuing the application of the phial wire to the rod, I exhauſted the natural quantity from the inſide ſurface of the ſame phials, or, as I call it, charg'd them *negatively*.

At length, while I was charging a phial by my glaſs globe, to repeat this experiment, my bells, of themſelves, ſtopt ringing, and, after ſome pauſe, began to ring again. —But now, when I approached the wire of the charg'd

phial to the rod, inſtead of the uſual ſtream that I expected from the wire to the rod, there was no ſpark; not even when I brought the wire and the rod to touch; yet the bells continued ringing vigorouſly; which prov'd to me, that the rod was then *poſitively* electrify'd, as well as the wire of the phial, and equally ſo; and conſequently, that the particular cloud then over the rod, was in the ſame poſitive ſtate. This was near the end of the guſt.

But this was a ſingle experiment, which however deſtroys my firſt too general concluſion, and reduces me to this: *That the clouds of a thunder-guſt are moſt commonly in a negative ſtate of electricity, but ſometimes in a poſitive ſtate.*

The latter I believe is rare; for tho' I ſoon after the laſt experiment, ſet out on a journey to *Boſton*, and was from home moſt part of the ſummer, which prevented my making farther trials and obſervations; yet Mr *Kinnerſley* returning from the iſlands juſt as I left home, purſu'd the experiments during my abſence, and informs me that he always found the clouds in the *negative* ſtate.

So that, for the moſt part, in thunder-ſtrokes, *'tis the earth that ſtrikes into the clouds, and not the clouds that ſtrike into the earth.*

Thoſe who are vers'd in electric experiments, will eaſily conceive, that the effects and appearances muſt be nearly the ſame in either caſe; the ſame exploſion, and the ſame flaſh between one cloud and another, and between the clouds

clouds and mountains, *&c.* the ſame rending of trees, walls, *&c.* which the electric fluid meets with in its paſſage, and the ſame fatal ſhock to animal bodies; and that pointed rods fix'd on buildings, or maſts of ſhips, and communicating with the earth or ſea, muſt be of the ſame ſervice in reſtoring the equilibrium ſilently between the earth and clouds, or in conducting a flaſh or ſtroke, if one ſhould be, ſo as to ſave harmleſs the houſe or veſſel: For points have equal power to throw off, as to draw on the electric fire, and rods will conduct up as well as down.

But tho' the light gain'd from theſe experiments makes no altration in the practice, it makes a conſiderable one in the theory. And now we as much need an hypotheſis to explain by what means the clouds become negatively, as before to ſhew how they became poſitively electrified.

I cannot forbear venturing ſome few conjectures on this occaſion: They are what occur to me at preſent; and tho' future diſcoveries ſhould prove them not wholly right, yet they may in the mean time be of ſome uſe, by ſtirring up the curious to make more experiments, and occaſion more exact diſquiſitions.

I conceive then, that this globe of earth and water, with its plants, animals and buildings, have, diffus'd throughout their ſubſtance, a quantity of the electric fluid, juſt as much as they can contain, which I call the *natural quantity*.

That this natural quantity is not the ſame in all kinds of common matter under the ſame dimenſions, nor in the

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ſame kind of common matter in all circumſtances; but a ſolid foot, for inſtance, of one kind of common matter, may contain more of the electric fluid than a ſolid foot of ſome other kind of common matter; and a pound weight of the ſame kind of common matter may, when in a rarer ſtate, contain more of the electric fluid than when in a denſer ſtate.

For the electric fluid, being attracted by any portion of common matter, the parts of that fluid (which have among themſelves a mutual repulſion) are brought ſo near to each other by the attraction of the common matter that abſorbs them, as that their repulſion is equal to the condenſing power of attraction in common matter; and then ſuch portion of common matter will abſorb no more.

Bodies of different kinds having thus attracted and abſorb'd what I call their *natural quantity*, *i. e.* juſt as much of the electric fluid as is ſuited to their circumſtances of denſity, rarity, and power of attracting, do not then ſhow any ſigns of electricity among each other.

And if more electric fluid be added to one of theſe bodies, it does not enter, but ſpreads on the ſurface, forming an atmoſphere; and then ſuch body ſhows ſigns of electricity.

I have in a former paper compar'd common matter to a ſponge, and the electric fluid to water: I beg leave once more to make uſe of the ſame compariſon, to illuſtrate farther my meaning in this particular.

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When a ſponge is ſomewhat condens'd by being ſqueez-ed between the fingers, it will not receive and retain ſo much water as when in its more looſe and open ſtate.

If *more* ſqueez'd and condens'd, ſome of the water will come out of its inner parts, and flow on the ſurface.

If the preſſure of the fingers be intirely removed, the ſponge will not only reſume what was lately forced out, but attract an additional quantity.

As the ſponge in its rarer ſtate will *naturally* attract and abſorb *more* water, and in its denſer ſtate will *naturally* attract and abſorb *leſs* water; we may call the quantity it attracts and abſorbs in either ſtate, its *natural quantity*, the ſtate being conſidered.

Now what the ſponge is to water, the ſame is water to the electric fluid.

When a portion of water is in its common denſe ſtate, it can hold no more electric fluid than it has; if any be added, it ſpreads on the ſurface.

When the ſame portion of water is rarify'd into vapour, and forms a cloud, it is then capable of receiving and abſorbing a much greater quantity; there is room for each particle to have an electric atmoſphere.

Thus water, in its rarify'd ſtate, or in the form of a cloud, will be in a negative ſtate of electricity; it will have leſs than its *natural quantity*; that is, leſs than it is naturally capable of attracting and abſorbing in that ſtate.

Such a cloud, then, coming ſo near the earth as to be within the ſtriking diſtance, will receive from the earth a

flaſh of the electric fluid; which flaſh, to ſupply a great extent of cloud, muſt ſometimes contain a very great quantity of that fluid.

Or ſuch a cloud, paſſing over woods of tall trees, may from the points and ſharp edges of their moiſt top leaves, receive ſilently ſome ſupply.

A cloud being by any means ſupply'd from the earth, may ſtrike into other clouds that have not been ſupply'd, or not ſo much ſupply'd; and thoſe to others, till an equilibrium is produc'd among all the clouds that are within ſtriking diſtance of each other.

The cloud thus ſupply'd, having parted with much of what it firſt receiv'd, may require and receive a freſh ſupply from the earth, or from ſome other cloud, which by the wind is brought into ſuch a ſituation as to receive it more readily from the earth.

Hence repeated, and continual ſtrokes and flaſhes till the clouds have all got nearly their natural quantity as clouds; or till they have deſcended in ſhowers, and are united again with this terraqueous globe, their original.

Thus thunder-clouds are generally in a negative ſtate of electricity compar'd with the earth, agreeable to moſt of our experiments; yet as by one experiment we found a cloud electris'd poſitively; I conjecture that, in that caſe, ſuch cloud, after having received what was, in its rare ſtate, only its *natural quantity*, became compreſs'd by the driving winds, or ſome other means, ſo that part of what it had abſorb'd was forc'd out, and form'd an electric atmoſphere

moſphere around it in its denſer ſtate. Hence it was capable of communicating poſitive electricity to my rod.

To ſhow that a body in different circumſtances of dilatation and contraction is capable of receiving and retaining more or leſs of the electric fluid on its ſurface, I would relate the following experiment. I placed a clean wine glaſs on the floor, and on it a ſmall ſilver can. In the can I put about three yards of braſs chain; to one end of which I faſtened a ſilk thread, which went right up to the cieling, where it paſſed over a pulley, and came down again to my hand, that I might at pleaſure draw the chain up out of the can, extending it till within a foot of the cieling, and let it gradually ſink into the can again.——From the cieling, by another thread of fine raw ſilk, I ſuſpended a ſmall light lock of cotton, ſo as that when it hung perpendicularly, it came in contact with the ſide of the can.——Then approaching the wire of a charged vial to the can, I gave it a ſpark, which flow'd round it in an electric atmoſphere; and the lock of cotton was repelled from the ſide of the can to the diſtance of about nine or ten inches. The can would not then receive another ſpark from the wire of the vial; but as I gradually drew up the chain, the atmoſphere of the can diminiſh'd by flowing over the riſing chain, and the lock of cotton accordingly drew nearer and nearer to the can; and then, if I again brought the vial wire near the can, it would receive another ſpark, and the cotton fly off again to its firſt diſtance; and thus, as the chain

was drawn higher, the can would receive more ſparks; becauſe the can and extended chain were capable of ſupporting a greater atmoſphere than the can with the chain gather'd up into its belly.——And that the atmoſphere round the can was diminiſhed by raiſing the chain, and increaſed again by lowering it, is not only agreeable to reaſon, ſince the atmoſphere of the chain muſt be drawn from that of the can, when it roſe, and returned to it again when it fell; but was alſo evident to the eye, the lock of cotton always approaching the can when the chain was drawn up, and receding when it was let down again.

Thus we ſee that increaſe of ſurface makes a body capable of receiving a greater electric atmoſphere: But this experiment does not, I own, fully demonſtrate my new hypotheſis; for the braſs and ſilver ſtill continue in their ſolid ſtate, and are not rarified into vapour, as the water is in clouds. Perhaps ſome future experiments on vapourized water may ſet this matter in a clearer light.

One ſeemingly material objection ariſes to the new hypotheſis, and it is this. If water, in its rarified ſtate, as a cloud, requires, and will abſorb more of the electric fluid than when in its denſe ſtate as water, why does it not acquire from the earth all it wants at the inſtant of its leaving the ſurface, while it is yet near, and but juſt riſing in vapour? To this difficulty I own I cannot at preſent give a ſolution ſatisfactory to myſelf: I thought,

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however, that I ought to ſtate it in its full force, as I have done, and ſubmit the whole to examination.

And I would beg leave to recommend it to the curious in this branch of natural philoſophy, to repeat with care and accurate obſervation, the experiments I have reported in this and former papers relating to *poſitive* and *negative* electricity, with ſuch other relative ones as ſhall occur to to them, that it may be certainly known whether the electricity communicated by a glaſs globe, be *really poſitive*. And alſo I would requeſt all who may have an opportunity of obſerving the recent effects of lightning on buildings, trees, *&c.* that they would conſider them particularly with a view to diſcover the direction. But in theſe examinations, this one thing is always to be underſtood, *viz.* that a ſtream of the electric fluid paſſing thro' wood, brick, metal, *&c.* while ſuch fluid paſſes in *ſmall quantity*, the mutually repulſive power of its parts is confined and overcome by the coheſion of the parts of the body it paſſes through, ſo as to prevent an exploſion; but when the fluid comes in a quantity too great to be confin'd by ſuch coheſion, it explodes, and rends or fuſes the body that endeavour'd to confine it. If it be wood, brick, ſtone, or the like, the ſplinters will flie off on that ſide where there is leaſt reſiſtance. And thus, when a whole is ſtruck thro' paſte-board by the electrify'd jar, if the ſurfaces of the paſte-board are not confin'd or compreſs'd, there will be a bur rais'd all round the hole on both ſides the paſteboard; but if one ſide be confin'd, ſo that the bur cannot be rais'd on that

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ſide, it will be all rais'd on the other, which way ſoever the fluid was directed. For the bur round the outſide of the hole, is the effect of the exploſion every way from the center of the ſtream, and not an effect of the direction.

In every ſtroke of lightning, I am of opinion that the ſtream of the electric fluid, moving to reſtore the equilibrium between the cloud and the earth, does always previouſly find its paſſage, and mark out, as I may ſay, its own courſe, taking in its way all the conductors it can find; ſuch as metals, damp walls, moiſt wood, &c. and will go conſiderably out of a direct courſe, for the ſake of the aſſiſtance of good conductors; and that, in this courſe, it is actually moving, tho' ſilently and imperceptibly, before the exploſion, in and among the conductors; which exploſion happens only when the conductors cannot diſcharge it as faſt as they receive it, by reaſon of their being incompleat, diſunited, too ſmall, or not of the beſt materials for conducting. Metalline rods, therefore, of ſufficient thickneſs, and extending from the higheſt part of an edifice to the ground, being of the beſt materials and compleat conductors, will, I think, ſecure the building from damage; either by reſtoring the equilibrium ſo faſt as to prevent a ſtroke, or by conducting it in the ſubſtance of the rod as far as the rod goes, ſo that there ſhall be no exploſion but what is above its point, between that and the clouds.

If it be aſk'd, what thickneſs of a metalline rod may be ſuppos'd ſufficient? In anſwer, I would remark, that five large glaſs jars, ſuch as I have deſcribed in my former papers,

pers, diſcharge a very great quantity of electricy, which nevertheleſs will be all conducted round the corner of a book, by the fine filleting of gold on the cover, it following the gold the fartheſt way about, rather than take the ſhorter courſe through the cover, that not being ſo good a conductor. Now in this line of gold the metal is ſo extremely thin as to be little more than the colour of gold, and on an octavo book is not in the whole an inch ſquare, and therefore not the 36th part of a grain according to M. *Reaumur*; yet 'tis ſufficient to conduct the charge of 5 large jars, and how many more I know not. Now, I ſuppoſe a wire of $\frac{1}{4}$ inch diameter to contain about 5000 times as much metal as there is in that gold line, and if ſo, it will conduct the charge of 25,000 ſuch glaſs jarrs, which is a quantity, I imagine, far beyond what was ever contain'd in any one ſtroke of natural lightning. But a rod of half an inch diameter would conduct four times as much as one of a quarter.

And with regard to conducting; tho' a certain thickneſs of metal be required to conduct a great quantity of electricity, and, at the ſame time, keep its own ſubſtance firm and unſeparated; and a leſs quantity, as a very ſmall wire for inſtance, will be deſtroy'd by the exploſion; yet ſuch ſmall wire will have anſwer'd the end of conducting that ſtroke, tho' it become incapable of conducting another. And conſidering the extream rapidity with which the electric fluid moves without exploding, when it has a free paſſage, or compleat metal communication, I ſhould think

think a vaſt quantity would be conducted in a ſhort time, either to or from a cloud, to reſtore its equilibrium with the earth, by means of a very ſmall wire; and therefore thick rods ſhould ſeem not ſo neceſſary.—However, as the quantity of lightning diſcharg'd in one ſtroke cannot well be meaſured, and, in different ſtrokes, is certainly very various, in ſome much greater than others; and as iron (the beſt metal for the purpoſe, being leaſt apt to fuſe) is cheap, it may be well enough to provide a larger canal to guide that impetuous blaſt, than we imagine neceſſary: For, though one middling wire may be ſufficient, two or three can do no harm. And time, with careful obſervations well compar'd, will at length point out the proper ſize to greater certainty.

Pointed rods erected on edifices may likewiſe often prevent a ſtroke, in the following manner. An eye ſo ſituated as to view horizontally the under ſide of a thunder cloud, will ſee it very ragged, with a number of ſeparate fragments, or petty clouds, one under another, the loweſt ſometimes not far from the earth. Theſe, as ſo many ſtepping-ſtones, aſſiſt in conducting a ſtroke between the cloud and a building. To repreſent theſe by an experiment, take two or three locks of fine looſe cotton, connect one of them with the prime conductor by a fine thread of two inches, (which may be ſpun out of the ſame lock by the fingers) another to that, and the third to the ſecond, by like threads.—Turn the globe, and you

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will ſee theſe locks extend themſelves towards the table, (as the lower ſmall clouds do towards the earth) being attracted by it: But on preſenting a ſharp point erect under the loweſt, it will ſhrink up to the ſecond, the ſecond to the firſt, and all together to the prime conductor, where they will continue as long as the point continues under them. May not, in like manner, the ſmall electriſed clouds, whoſe equilibrium with the earth is ſoon reſtor'd by the point, riſe up to the main body, and by that means occaſion ſo large a vacancy, as that the grand cloud cannot ſtrike in that place?

Theſe thoughts, my dear friend, are many of them crude and haſty; and if I were merely ambitious of acquiring ſome reputation in philoſophy, I ought to keep them by me, till corrected and improved by time and farther experience. But ſince even ſhort hints, and imperfect experiments in any new branch of ſcience, being communicated, have oftentimes a good effect, in exciting the attention of the ingenious to the ſubject, and ſo become the occaſion of more exact diſquiſitions, and more compleat diſcoveries. You are at liberty to communicate this paper to whom you pleaſe; it being of more importance that knowledge ſhould increaſe, than that your friend ſhould be thought an accurate philoſopher.

LETTER XIII.

FROM

BENJAMIN FRANKLIN, *Esq*; at *Philadelphia*.

TO

PETER COLLINSON, *Esq*; F. R. S. *at London*.

SIR, *April* 18, 1754.

SINCE *September* last, having been abroad on two long journeys, and otherwise much engag'd, I have made but few observations on the *positive* and *negative* state of electricity in the clouds. But Mr *Kinnersley* kept his rod and bells in good order, and has made many.

Once this winter the bells rang a long time, during a fall of snow, tho' no thunder was heard or lightning seen. Sometimes the flashes and cracks of the electric matter between bell and bell were so large and loud, as to be heard all over the house: but by all his observations, the clouds were constantly in a negative state, till about six weeks ago, when he found them once to change in a few minutes from the negative to the positive. About a fort-

night

night after that he made another obſervation of the ſame kind; and laſt *Monday* afternoon, the wind blowing hard at S. E. and veering round to N. E. with many thick driving clouds, there were five or ſix ſucceſſive changes from negative to poſitive, and from poſitive to negative, the bells ſtopping a minute or two between every change. Beſides the methods mentioned in my paper of *September* laſt, of diſcovering the electrical ſtate of the clouds, the following may be us'd. When your bells are ringing, paſs a rubb'd tube by the edge of the bell, connected with your pointed rod: if the cloud is then in a negative ſtate, the ringing will ſtop; if in a poſitive ſtate, it will continue, and perhaps be quicker. Or, ſuſpend a very ſmall cork-ball by a fine ſilk thread, ſo that it may hang cloſe to the edge of the rod-bell: then whenever the bell is electrified, whether poſitively or negatively, the little ball will be repell'd, and continue at ſome diſtance from the bell. Have ready a round-headed glaſs ſtopper of a decanter, rub it on your ſide 'till it is electrified, then preſent it to the cork-ball. If the electricity in the ball is poſitive, it will be repell'd from the glaſs ſtopper as well as from the bell. If negative, it will fly to the ſtopper.

REMARKS
On the Abbe NOLLET's
LETTERS on ELECTRICITY.

TO

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

BY

Mr DAVID COLDEN, of *New-York*,

SIR, *Coldenham*, in *N. York*, *Dec.* 4, 1753.

IN considering the Abbé *Nollet*'s letters to Mr *Franklin*, I am obliged to pass by all the experiments which are made with, or in, bottles hermetically sealed, or exhausted of air; because, not being able to repeat the experiments, I could not second any thing, which occurs to me thereon, by experimental proof. Wherefore, the first point wherein I can dare to give my opinion, is in the Abbe's 4th letter, *p.* 66, where he undertakes to prove, that the electric matter passes from one surface to another through the intire thickness of the glass: He takes Mr *Franklin*'s experiment of the magical picture, and writes thus of it. "When you electrise a pane of glass coated

on

" on both ſides with metal, it is evident that whatever is " placed on the ſide oppoſite to that which receives the " electricity from the conductor, receives alſo an evident " electrical virtue." Which Mr *Franklin* ſays, is that equal quantity of electric matter, driven out of this ſide, by what is received from the conductor on the other ſide; and which will continue to give an electrical virtue, to any thing in contact with it, till it is entirely diſcharged of its electrical fire. To which the Abbé thus objects: " Tell me, ſays he, I pray you, how much time is ne- " ceſſary for this pretended diſcharge? I can aſſure you, " that after having maintain'd the electriſation for hours, " this ſurface, which ought, as it ſeems to me, to be en- " tirely diſcharged of its electrical matter, conſidering ei- " ther the vaſt number of ſparks that were drawn from it, " or the time that this matter had been expoſed to the action " of the expulſive cauſe; this ſurface, I ſay, appeared ra- " ther better electriſed thereby, and more proper to pro- " duce all the effects of an actual electric body." *p.* 68.

The Abbé does not tell us what thoſe effects were: all the effects I could never obſerve, and thoſe that are to be obſerved can eaſily be accounted for, by ſuppoſing that ſide to be entirely deſtitute of electric matter. The moſt ſenſible effect of a body charged with electricity is, that when you preſent your finger to it, a ſpark will iſſue from it to your finger: Now when a phial, prepared for the *Leyden* experiment, is hung to the gun-barrel or prime-

con-

conductor, and you turn the globe in order to charge it; as ſoon as the electric matter is excited, you can obſerve a ſpark to iſſue from the external ſurface of the phial to your finger; which, Mr *Franklin* ſays, is the natural electric matter of the glaſs driven out by that received by the inner ſurface from the conductor. If it be only drawn out by ſparks, a vaſt number of them may be drawn; but if you take hold of the external ſurface with your hand, the phial will ſoon receive all the electric matter it is capable of, and the outſide will then be entirely deſtitute of its electric matter, and no ſpark can be drawn from it by the finger: here then is a want of that effect which all bodies, charged with electricity, have. Some of the effects of an electric body, which I ſuppoſe the Abbé has obſerved in the exterior ſurface of a charged phial, are that all light bodies are attracted by it. This is an effect which I have conſtantly obſerved, but do not think that it proceeds from an attractive quality in the exterior ſurface of the phial, but in thoſe light bodies themſelves, which ſeem to be attracted by the phial. It is a conſtant obſervation, that when one body has a greater charge of electric matter in it than another (that is in proportion to the quantity they will hold) this body will attract that which has leſs: Now, I ſuppoſe, and it is a part of Mr *Franklin*'s ſyſtem, that all thoſe light bodies which appear to be attracted, have more electric matter in them than the external ſurface of the phial has, wherefore they endeavour to attract the

phial

phial to them, which is too heavy to be moved by the ſmall degree of force they exert, and yet being greater than their own weight, moves them to the phial. The following experiment will help the imagination in conceiving this. Suſpend a cork ball, or a feather by a ſilk thread and electriſe it, then bring this ball nigh to any fixed body, and it will appear to be attracted by that body, for it will fly to it: Now, by the conſent of electricians, the attractive cauſe is in the ball itſelf, and not in the fixed body to which it flies: This is a ſimilar caſe with the apparent attraction of light bodies, to the external ſurface of a charged phial.

The Abbé ſays, *p.* 69. "that he can electriſe a hundred men, ſtanding on wax, if they hold hands, and if one of them touch one of theſe ſurfaces (the exterior) with the end of his finger": This I know he can, while the phial is charging, but after the phial is charged I am as certain he cannot: That is, hang a phial, prepared for the *Leyden* experiment, to the conductor, and let a man, ſtanding on the floor, touch the coating with his finger, while the globe is turn'd, till the electric matter ſpews out of the hook of the phial, or ſome part of the conductor, which I take to be the certaineſt ſign that the phial has received all the electric matter it can: after this appears, let the man, who before ſtood on the floor, ſtep on a cake of wax, where he may ſtand for hours, and the globe all that time turned, and yet have no appearance of being electriſed. After

ter the electric matter was ſpewed out as above from the hook of a phial, prepared for the *Leyden* experiment, I hung another phial, in like manner prepared, to a hook fixed in the coating of the firſt, and held this other phial in my hand; now if there was any electric matter tranſmitted thro' the glaſs of the firſt phial, the ſecond one would certainly receive and collect it; but having kept the phials in this ſituation for a conſiderable time, during which the globe was continually turned, I could not perceive that the ſecond phial was in the leaſt charged, for when I touched the hook with my finger, as in the *Leyden* experiment, I did not feel the leaſt commotion, nor perceive any ſpark to iſſue from the hook.

I likewiſe made the following experiment. Having charged two phials (prepared for the *Leyden* experiment) through their hooks; two perſons took each one of theſe phials in their hand, one held his phial by the coating, the other by the hook, which he could do by removing the communication from the bottom before he took hold of the hook. Theſe perſons placed themſelves, one on each ſide of me, while I ſtood on a cake of wax, and took hold of the hook of that phial which was held by its coating (upon which a ſpark iſſued, but the phial was not diſcharged, as I ſtood on wax) keeping hold of the hook, I touched the coating of the phial that was held by its hook with my other hand; upon which there was a large ſpark to be ſeen between my finger and the coating, and both phials were inſtantly

inſtantly diſcharged. If the Abbé's opinion be right, that the exterior ſurface, communicating with the coating, is charged, as well as the interior, communicating with the hook; how can I, who ſtand on wax, diſcharge both theſe phials, when it is well known I could not diſcharge one of them ſingly? Nay, ſuppoſe I have drawn the electric matter from both of them, what becomes of it? For I appear to have no additional quantity in me when the experiment is over, and I have not ſtirr'd off the wax: Wherefore this experiment fully convinces me, that the exterior ſurface is not charged; and not only ſo, but that it wants as much electric matter as the inner has of exceſs: For by this ſuppoſition, which is a part of Mr *Franklin*'s ſyſtem, the above experiment is eaſily accounted for, as follows:

When I ſtand on wax, my body is not capable of receiving all the electric matter from the hook of one phial, which it is ready to give; neither can it give as much to the coating of the other phial as it is ready to take, when one is only

applied to me: But when both are applied, the coating takes from one what the hook gives: Thus I receive the fire from the firſt phial at B, the exterior ſurface of which is ſupplied from the hand at A: I give the fire to the ſecond phial at C, whoſe interior ſurface is diſcharged by the hand at D. This diſcharge at D may be made evident by receiving that fire into the hook of a third phial, which is done thus: In place of taking the hook of the ſecond phial in your hand, run the wire of a third phial, prepared as for the *Leyden* experiment, through it, and hold this third phial in your hand, the ſecond one hanging to it, by the ends of the hooks run through each other: When the experiment is performed, this third phial receives the fire at D, and will be charged. When this experiment is conſidered, I think, it muſt fully prove that the exterior ſurface of a charged phial wants electric matter, while the inner ſurface has an exceſs of it. One thing more, worthy of notice in this experiment is, that I feel no commotion or ſhock in my arms, tho' ſo great a quantity of electric matter paſſes through them inſtantaneouſly: I only feel a prickling in the ends of my fingers. This makes me think the Abbé has miſtook, when he ſays, that there is no difference between the ſhock felt in performing the *Leyden* experiment, and the prickling felt on drawing ſimple ſparks, except that of greater to leſs. In the laſt experiment, as much electric matter went through my arms, as would have given me a very ſenſible ſhock, had there been an immediate com-

mu-

munication, by my arms, from the hook to the coating of the ſame phial; becauſe when it was taken into a third phial, and that phial diſcharged ſingly thro' my arms, it gave me a ſenſible ſhock. If theſe experiments prove that the electric matter does not paſs through the intire thickneſs of the glaſs; it is a neceſſary conſequence that it muſt always come out where it enter'd.

The next thing I meet with, is in the Abbé's fifth letter *p.* 88, where he differs from Mr *Franklin*, who thinks that the whole power of giving a ſhock is in the glaſs itſelf, and not in the non-electrics in contact with it. The experiments which Mr *Franklin* gave to prove this opinion in his *Experiments and Obſervations on Electricity*, *Letter* III. *p.* 24. convinced me that he was in the right; and what the Abbé has aſſerted in contradiction thereto, has not made me think otherwiſe. The Abbé perceiving, as I ſuppoſe, that the experiments, as Mr *Franklin* had perform'd them, muſt prove his aſſertion; alters them without giving any reaſon for it, and makes them in a manner that proves nothing. Why will he have the phial, into which the water is to be decanted from a charged phial, held in a man's hand? If the power of giving a ſhock is in the water contain'd in the phial, it ſhould remain there tho' decanted into another phial, ſince no non-electric body touch'd it to take that power off. The phial being placed on wax is no objection, for it cannot take the power from the water, if it had any, but it is a neceſſary

means to try the fact; whereas, that phial's being charged when held in a man's hand, only proves that water will conduct the electric matter. The Abbé owns, *p.* 94. that he had heard this remarked, but says, Why is not a conductor of electricity an electric subject? This is not the question; Mr *Franklin* never said that water was not an electric subject; he said, that the power of giving a shock was in the glass, and not in the water; and this, his experiments, fully prove; so fully, that it may appear impertinent to offer any more: Yet as I do not know that the following has been taken notice of by any body before, my inserting of it in this place may be excused. It is this: Hang a phial, prepared for the *Leyden* experiment, to the conductor, by its hook, and charge it, which done, remove the communication from the bottom of the phial. Now the conductor shews evident signs of being electrised; for if a thread be tied round it, and it s ends left about two inches long, they will extend themselves out like a pair of horns; but if you touch the conductor, a spark will issue from it, and the threads will fall, nor does the conductor shew the least sign of being electrised after this is done. I think that by this touch, I have taken out all the charge of electric matter that was in the conductor, the hook of the phial, and water or filings of iron contain'd in it; which is no more than we see all non-electric bodies will receive; yet the glass of the phial retains its power of giving a shock, as any one will find

find that pleaſes to try. This experiment fully evidences, that the water in the phial contains no more electric matter than it would do in an open baſon, and has not any of that great quantity which produces the ſhock, and is only retain'd by the glaſs. If after the ſpark is drawn from the conductor, you touch the coating of the phial (which all this while is ſuppoſed to hang in the air, free from any non-electric body) the threads on the conductor will inſtantly ſtart up, and ſhew that the conductor is electriſed. It receives this electriſation from the inner ſurface of the phial, which, when the outer ſurface can receive what it wants from the hand applied to it, will give as much as the bodies in contact with it can receive, or, if they be large enough, all that it has of exceſs. It is diverting to ſee how the threads will riſe and fall by touching the coating and conductor of the phial alternately. May it not be that the difference between the charged ſide of the glaſs, and the outer or emptied ſide, being leſſen'd by touching the hook or the conductor; the outer ſide can receive from the hand which touched it, and by its receiving the inner ſide cannot retain ſo much; and for that reaſon ſo much as it cannot retain electriſes the water, or filings and conductor: For it ſeems to be a rule, that the one ſide muſt be emptied in the ſame proportion that the other is fill'd: Tho' this from experiment appears evident, yet it is ſtill a myſtery not to be accounted for.

I am

I am, in many places of the Abbé's book, ſurpriſed to find that experiments have ſucceeded ſo differently at *Paris* from what they did with Mr *Franklin*, and as I have always obſerv'd them to do. The Abbé, in making experiments to find the difference between the two ſurfaces of a charged glaſs, will not have the phial placed on wax: For, ſays he, don't you know that being placed on a body originally electric, it quickly loſes its virtue? I cannot imagine what ſhould have made the Abbé think ſo; it certainly is contradictory to the notions commonly received of electrics per ſe; and by experiment I find it entirely otherwiſe: For having ſeveral times left a charged phial, for that purpoſe, ſtanding on wax for hours, I found it to retain as much of its charge as another that ſtood at the ſame time on a table. I left one ſtanding on wax from 10 o'clock at night till 8 next morning, when I found it to retain a ſufficient quantity of its charge, to give me a ſenſible commotion in my arms, though the room in which the phial ſtood had been ſwept in that time, which muſt have rais'd much duſt to facilitate the diſcharge of the phial.

I find that a cork ball ſuſpended between two bottles, the one fully and the other but little charged, will not play between them, but is driven into a ſituation that makes a triangle with the hooks of the phials; though the Abbé has aſſerted the contrary of this, *p.* 101, in order to account for the playing of a cork ball between the

wire

wire thruſt into the phial, and one that riſes up from its coating. The phial which is leaſt charged muſt have more electric matter given to it, in proportion to its bulk, than the cork ball receives from the hook of the full phial.

The Abbé ſays, *p.* 103, "that a piece of metal leaf "hung to a ſilk thread and electriſed, will be repell'd by "the bottom of a charged phial held by its hook in the "air;" This I find conſtantly otherwiſe, it is with me always firſt attracted and then repelled: It is neceſſary in charging the leaf to be careful that it does not fly off to ſome non-electric body, and ſo diſcharge itſelf when you think it is charged; it is difficult to keep it from flying to your own wriſt, or to ſome part of your body.

The Abbé, *p.* 108, ſays, "that it is not impoſſible, as "Mr *Franklin* ſays it is, to charge a phial while there is a "communication form'd between its coating and its hook". I have always found it impoſſible to charge ſuch a phial ſo as to give a ſhock: Indeed if it hang on the conductor without a communication from it, you may draw a ſpark from it as you may from any body that hangs there, but this is very different from being charged in ſuch a manner as to give a ſhock. The Abbé, in order to account for the little quantity of electric matter that is to be found in the phial, ſays, "that it rather follows the metal than the "glaſs, and that it is ſpewed out into the air from the coating "of the phial". I wonder how it comes not to do ſo too,

when

when it ſifts through the glaſs and charges the exterior ſurface, according to the Abbé's ſyſtem!

The Abbé's objections againſt Mr *Franklin*'s two laſt experiments, I think, have little weight in them: He ſeems, indeed, much at a loſs what to ſay, wherefore he taxes Mr *Franklin* with having conceal'd a material part of the experiment; a thing too mean for any gentleman to be charged with, who has not ſhewn as great a partiality in relating experiments, as the Abbé has done.

ELEC-

ELECTRICAL EXPERIMENTS,

With an Attempt to account for their

SEVERAL PHÆNOMENA;

Together with

Some Obſervations on *Thunder-Clouds*,

In further Confirmation of Mr FRANKLIN's Obſervations on the poſitive and negative electrical State of the Clouds, by JOHN CANTON, M. A. and F. R. S.

Dec. 6, 1753.

EXPERIMENT I.

FROM the cieling, or any convenient part of a room, let two cork-balls, each about the bigneſs of a ſmall pea, be ſuſpended by linen threads of eight or nine inches in length, ſo as to be in contact with each other. Bring the excited glaſs tube under the balls,

and they will be ſeparated by it, when held at the diſtance of three or four feet; let it be brought nearer, and they will ſtand farther apart; intirely withdraw it, and they will immediately come together. This experiment may be made with very ſmall braſs balls hung by ſilver wire; and will ſucceed as well with ſealing-wax made electrical, as with glaſs.

EXPERIMENT II.

If two cork-balls be ſuſpended by dry ſilk threads, the excited tube muſt be brought within eighteen inches before they will repel each other; which they will continue to do, for ſome time, after the tube is taken away.

As the balls in the firſt experiment are not inſulated, they cannot properly be ſaid to be electrified: but when they hang within the atmoſphere of the excited tube, they may attract and condenſe the electrical fluid round about them, and be ſeparated by the repulſion of its particles. It is conjectur'd alſo, that the balls at this time contain leſs than their common ſhare of the electrical fluid, on account of the repelling power of that which ſurrounds them; tho' ſome, perhaps, is continually entering and paſſing thro' the threads. And if that be the caſe, the reaſon is plain, why the balls hung by ſilk, in the ſecond experiment, muſt be in a much more denſe part of the atmoſphere of the tube, before they will repel each other. At the approach of an excited ſtick of wax to the balls, in the firſt experiment, the electrical fire is ſuppoſed to come

come through the threads into the balls, and be condenſed there, in its paſſage towards the wax: for, according to Mr *Franklin*, excited glaſs *emits* the electrical fluid, but excited wax *receives* it.

EXPERIMENT III.

Let a tin tube, of four or five feet in length, and about two inches inches in diameter, be inſulated by ſilk; and from one end of it let the cork-balls be ſuſpended by linen threads. Electrify it, by bringing the excited glaſs tube near the other end, ſo as that the balls may ſtand an inch and an half, or two inches apart: then, at the approach of the excited tube, they will by degrees loſe their repelling power, and come into contact; and as the tube is brought ſtill nearer, they will ſeparate again to as great a diſtance as before: in the return of the tube they will approach each other till they touch, and then repel as at firſt. If the tin-tube be electrified by wax, or the wire of a charg'd phial, the balls will be affected in the ſame manner at the approach of excited wax, or the wire of the phial.

EXPERIMENT IV.

Electrify the cork-balls as in the laſt experiment by glaſs; and at the approach of an excited ſtick of wax their repulſion will be increaſed. The effect will be the ſame, if the excited glaſs be brought towards them, when they have been electrified by wax.

The bringing the excited glaſs to the end, or edge of

the tin-tube, in the third experiment, is ſuppos'd to electrify it poſitively, or to add to the electrical fire it before contained; and therefore ſome will be running off through the balls, and they will repel each other. But at the approach of excited glaſs, which likewiſe *emits* the electrical fluid, the diſcharge of it from the balls will be diminiſh'd; or part will be driven back, by a force acting in a contrary direction; and they will come nearer together. If the tube be held at ſuch a diſtance from the balls, that the exceſs of the denſity of the fluid round about them, above the common quantity in air, be equal to the exceſs of the denſity of that within them, above the common quantity contain'd in cork; their repulſion will be quite deſtroy'd. But if the tube be brought nearer; the fluid without, being more denſe than that within the balls, it will be attracted by them, and they will recede from each other again.

When the apparatus has loſt part of its natural ſhare of this fluid, by the approach of excited wax to one end of it, or is electrified negatively; the electrical fire is attracted and imbib'd by the balls to ſupply the deficiency; and that more plentifully at the approach of excited glaſs, or a body poſitively electrified, than before; whence the diſtance between the balls will be increaſed, as the fluid ſurrounding them is augmented. And in general, whether by the approach or receſs of any body; if the difference between the denſity of the internal and external fluid

be

be increaſed, or diminiſhed; the repulſion of the balls will be increaſed, or diminiſhed, accordingly.

EXPERIMENT V.

When the inſulated tin tube is not electrified, bring the excited glaſs tube towards the middle of it, ſo as to be nearly at right angles with it, and the balls at the end will repel each other; and the more ſo, as the excited tube is brought nearer. When it has been held a few ſeconds, at the diſtance of about ſix inches, withdraw it, and the balls will approach each other till they touch; and then ſeparating again, as the tube is moved farther off, will continue to repel when it is taken quite away. And this repulſion between the balls will be increaſed by the approach of excited glaſs, but diminiſhed by excited wax; juſt as if the apparatus had been electrified by wax, after the manner deſcribed in the third experiment.

EXPERIMENT VI.

Inſulate two tin tubes, diſtinguiſhed by *A* and *B*, ſo as to be in a line with each other, and about half an inch apart; and at the remote end of each, let a pair of cork balls be ſuſpended. Towards the middle of *A*, bring the excited glaſs tube; and holding it a ſhort time, at the diſtance of a few inches, each pair of balls will be obſerved to ſeparate: withdraw the tube, and the balls of *A* will come together, and then repel each other again; but thoſe of *B* will hardly be affected. By the approach of the excited

cited glaſs tube, held under the balls of *A*, their repulſion will be increaſed: but if the tube be brought, in the ſame manner, towards the balls of *B*, their repulſion will be diminiſhed.

In the fifth experiment, the common ſtock of electrical matter in the tin tube, is ſuppoſed to be attenuated about the middle, and to be condenſed at the ends, by the repelling power of the atmoſphere of the excited glaſs tube, when held near it. And perhaps the tin tube may loſe ſome of its natural quantity of the electrical fluid, before it receives any from the glaſs; as that fluid will more readily run off from the ends or edges of it, than enter at the middle: and accordingly, when the glaſs tube is withdrawn, and the fluid is again equally diffuſed through the apparatus, it is found to be electrified negatively: For excited glaſs brought under the balls will increaſe their repulſion.

In the ſixth experiment, part of the fluid driven out of one tin tube enters the other; which is found to be electrified poſitively, by the decreaſing of the repulſion of its balls, at the approach of excited glaſs.

EXPERIMENT VII.

Let the tin tube, with a pair of balls at one end, be placed three feet at leaſt from any part of the room, and the air render'd very dry by means of a fire: electrify the apparatus to a conſiderable degree; then touch the tin tube with a finger, or any other conductor, and the balls will,

will, notwithſtanding, continue to repel each other; tho' not at ſo great a diſtance as before.

The air ſurrounding the apparatus to the diſtance of two or three feet, is ſuppoſed to contain more or leſs of the electrical fire, than its common ſhare, as the tin tube is electrified poſitively, or negatively; and when very dry, may not part with its overplus, or have its deficiency ſupplied ſo ſuddenly, as the tin; but may continue to be electrified, after that has been touch'd for a conſiderable time.

EXPERIMENT VIII.

Having made the Torricellian vacuum about five feet long, after the manner deſcribed in the *Philoſophical Tranſactions*, Vol. xlvii. p. 370. if the excited tube be brought within a ſmall diſtance of it, a light will be ſeen through more than half its length: which ſoon vaniſhes, if the tube be not brought nearer; but will appear again, as that is moved farther off. This may be repeated ſeveral times, without exciting the tube afreſh.

This experiment may be conſider'd as a kind of ocular demonſtration of the truth of Mr *Franklin*'s hypotheſis; that when the electrical fluid is condenſed on one ſide of thin glaſs, it will be repelled from the other, if it meets with no reſiſtance. According to which, at the approach of the excited tube, the fire is ſuppoſed to be repelled from the inſide of the glaſs ſurrounding the vacuum, and to be

carried

carried off through the columns of mercury; but, as the tube is withdrawn, the fire is ſuppoſed to return.

EXPERIMENT IX.

Let an excited ſtick of wax, of two feet and an half in length, and about an inch in diameter, be held near its middle. Excite the glaſs tube, and draw it over one half of it; then, turning it a little about its axis, let the tube be excited again, and drawn over the ſame half; and let this operation be repeated ſeveral times: then will that half deſtroy the repelling power of balls electrified by glaſs, and the other half will increaſe it.

By this experiment it appears, that wax alſo may be electrified poſitively and negatively. And it is probable, that all bodies whatſoever may have the quantity they contain of the electrical fluid, increaſed, or diminiſhed. The clouds, I have obſerved, by a great number of experiments, to be ſome in a poſitive, and others in a negative ſtate of electricity. For the cork balls, electrified by them, will ſometimes cloſe at the approach of excited glaſs; and at other times be ſeparated to a greater diſtance. And this change I have known to happen five or ſix times in leſs than half an hour; the balls coming together each time, and remaining in contact a few ſeconds, before they repel each other again. It may likewiſe eaſily be diſcover'd, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by a poſi-

a poſitive one: and by whichſoever it be electrified, ſhould that cloud either part with its overplus, or have its deficiency ſupplied ſuddenly, the apparatus will loſe its electricity: which is frequently obſerved to be the caſe, immediately after a flaſh of lightning. Yet when the air is very dry, the apparatus will continue to be electriſed for ten minutes, or a quarter of an hour, after the clouds have paſſed the zenith; and ſometimes till they appear more than half-way towards the horizon. Rain, eſpecially when the drops are large, generally brings down the electrical fire: and hail, in ſummer, I believe never fails. When the apparatus was laſt electrified, it was by the fall of thawing ſnow; which happened ſo lately, as on the 12th of *November*; that being the twenty-ſixth day, and ſixty-firſt time, it has been electrified, ſince it was firſt ſet up; which was about the middle of *May*. And as *Fahrenheit*'s thermometer was but ſeven degrees above freezing, it is ſuppoſed the winter will not intirely put a ſtop to obſervations of this ſort. At *London*, no more than two thunderſtorms have happened during the whole ſummer: and the apparatus was ſometimes ſo ſtrongly electrified in one of them, that the bells, which have been frequently rung by the clouds, ſo loud as to be heard in every room of the houſe (the doors being open), were ſilenced by the almoſt conſtant ſtream of denſe electrical fire, between each bell and the braſs ball, which would not ſuffer it to ſtrike.

I ſhall conclude this paper, already too long, with the following queries:

1. May not air, ſuddenly rarefied, give electrical fire to, and air ſuddenly condenſed, receive electrical fire from, clouds and vapours paſſing through it?

2. Is not the *aurora borealis*, the flaſhing of electrical fire from poſitive, towards negative clouds at a great diſtance, through the upper part of the atmoſphere, where the reſiſtance is leaſt?

APPENDIX.

AS Mr *Franklin*, in a former letter to Mr *Collinſon*, mentioned his intending to try the power of a very ſtrong electrical ſhock upon a turkey, that gentleman accordingly has been ſo very obliging as to ſend an account of it, which is to the following purpoſe.

He made firſt ſeveral experiments on fowls, and found, that two large thin glaſs jars gilt, holding each about ſix gallons, and ſuch as I mentioned I had employed in the laſt paper I laid before you on this ſubject, were ſufficient, when fully charged, to kill common hens outright; but the turkeys, though thrown into violent convulſions, and then, lying as dead for ſome minutes, would recover in leſs than a quarter of an hour. However, having added three other ſuch to the former two, though not fully charged, he killed a turkey of about ten pounds weight, and believes that they would have killed a much larger. He conceited, as himſelf ſays, that the birds kill'd in this manner eat uncommonly tender.

In making theſe experiments, he found, that a man could, without great detriment, bear a much greater ſhock than he imagined: for he inadvertently received the ſtroke of two of theſe jars through his arms and body, when they were very near fully charged. It ſeemed to him an univerſal blow throughout the body from head to foot,

and

and was followed by a violent quick trembling in the trunk, which went off gradually in a few ſeconds. It was ſome minutes before he could recollect his thoughts, ſo as to know what was the matter; for he did not ſee the flaſh, tho' his eye was on the ſpot of the prime conductor, from whence it ſtruck the back of his hand; nor did he hear the crack, though the by-ſtanders ſaid it was a loud one; nor did he particularly feel the ſtroke on his hand, tho' he afterwards found it had raiſed a ſwelling there, of the bigneſs of half a ſwan-ſhot, or piſtol-bullet. His arms and the back of his neck felt ſomewhat numbed the remainder of the evening, and his breaſt was ſore for a week after, as if it had been bruiſed. From this experiment may be ſeen the danger, even under the greateſt caution, to the operator, when making theſe experiments with large jars; for it is not to be doubted, but ſeveral of theſe fully charged would as certainly, by increaſing them, in proportion to the ſize, kill a man, as they before did a turkey.

N. B. The original of this letter, which was read at the Royal Society, has been miſlaid.

FINIS.

CONTENTS of all the PARTS.

LETTER

CONTENTS.

APPENDIX.

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